avoided at all costs. The choice lies between the first horn, as Professor Newcomb states it; or

An acceleration of the Earth's orbital motion must be accepted which will make the eclipses agree well with the most probable interpretation of the records.

6. I am basing my hypothesis of a secular acceleration for the Sun upon the acceptance, and not upon the rejection, of the theoretical position of the node.

7. There are contemporary records of very few eclipses. Great numbers of partial eclipses must have occurred. I attach great weight to the fact that a very simple assumption makes the six eclipses considered central at specified places. In my view we have heard of those eclipses, because they were striking phenomena worth recording. The lunar eclipses and the transits of *Mercury* are not very searching tests, but so far as they go they confirm the assumption referred to.

8. I have not rejected the results of gravitational theory. I at once acquiesced in the following argument as soon as it was presented to me: *

i. The motion of the node accords with theory now to well within 20″ per century, which is therefore the extreme value to be assigned to the action of unknown causes.

ii. Therefore the position of the node twenty-five centuries ago must be considered known to within 500″.

I presume Professor Newcomb does not expect me to consider such an argument as the following conclusive:—

i. No theoretical reason is known for a change in the Earth's mean motion.

ii. Therefore the Earth's mean motion must be constant.

To expect this would be equivalent to saying that theory must always precede observation.

Finally I would say that the question is one of evidence. There must be a degree of evidence that would be considered sufficient to establish the fact. I have produced a considerable amount of evidence, all pointing one way; and the conclusion seems to me highly probable.

On the Transits of Mercury, 1677–1881. By P. H. Cowell.

In this paper I examine the transits of *Mercury* to determine whether they support or contradict the supposition of a secular acceleration of 4″ in the Earth's orbital motion that I have deduced from ancient solar eclipses.

It is well known that the secular acceleration of the Moon cannot be determined from modern observations on account of the term of long period, on which theory is altogether silent;

* In my first paper on ancient solar eclipses I had overlooked Professor Brown's paper on the secular accelerations in the *Monthly Notices* of 1897.

while observation can only give a very rough approximation to its value.

In like manner the assumption that no unknown long-period terms exist in the motion of the Earth or *Mercury* necessarily underlies the present paper, and hence no secular accelerations could have in the first instance been deduced from a discussion of the transits. I show, however, that with the above suppositions the transits of *Mercury* do indicate a secular acceleration for the Earth, but not for *Mercury*. The amount here deduced is $2''\cdot5$ for the Earth, in fair accordance with the $4''$ deduced from ancient eclipses, considering the short extent of time covered by the transits of *Mercury*. For *Mercury* the secular acceleration obtained is $-0''\cdot5$. If this be treated as accidental, and put equal to zero in the equations of condition, the secular acceleration for the Earth goes up to $3''\cdot2$, in still better accordance with the result from ancient eclipses.

The phenomenon, accounted for to a great extent in this paper by the supposition of a secular acceleration for the Sun, is twice recognised by Professor Newcomb in his discussion of the transits of *Mercury* (*Astron. Papers*, vol. i. p. vi). On p. 450 he finds the corrections required by the tabular times to be closely represented by $+60^s \cdot T^2$, a quantity far too large to be attributed to tidal retardation of the Earth's rotation. Again, on p. 460 he finds that the phenomena can be numerically explained by supposing a long-period inequality in the Earth's rotation sufficient to account for three tenths of the errors of the Moon. That there is something to explain may therefore be taken on the authority of Professor Newcomb. My present point is that a secular acceleration of the Earth does explain the phenomenon with reasonable accuracy, and that the hypothesis is not an arbitrary one invented *ad hoc*, but an hypothesis to which I had already been led by other evidence.

The material I use consists of the internal contacts only. Professor Newcomb (*Astron. Papers*, vol. i. pp. 457–8) has given equations of conditions resulting from fourteen November transits and six May transits, and has assigned weights to each equation of condition. I accept his weights unaltered. I remove, however, from his equations of condition every unknown quantity but V for the November transits and W for the May transits. To do this I substitute $k = 0$ in his equations, and for N, M, S the values that he has obtained on p. 459.

$$\begin{aligned} N &= -0''\cdot16 + 0''\cdot28T \\ M &= +0\cdot15 \\ S &= -0\cdot04 \end{aligned}$$

where T is the time measured in centuries from 1820.

It will be seen in Professor Newcomb's paper that N involves a correction to the node and S to the semi-diameters, and that

consequently relatively to V, W (which involve corrections to the longitudes), N and S change sign between second and third contacts. When, therefore, both second and third contacts are employed the values of V, W are very little affected by possible errors in N, S. Again, M involves a correction to the mass of *Venus*. There is not much uncertainty in this correction, and relatively to V, W its effect, though the same at both second and third contacts, changes sign twice or thrice during the period under discussion. Consequently the values of V, W will be very little affected by assuming a value for M.

The quantity k that I put equal to zero was introduced by Professor Newcomb on the hypothesis that the errors of the Moon might be attributed to irregularities in the rotation of the Earth. His solution $k = +0{\cdot}295$ means that three tenths of the errors of the Moon appear, on solution of the equations of condition, to be attributable to this cause. The choice, however, obviously lies between $k = 0$ (uniform rotation, the explanation of the Moon's errors to be sought elsewhere), and $k = 1$ (Moon's errors entirely explained by irregularities of rotation). An intermediate value of k merely replaces a single difficulty by two, and Professor Newcomb regards $k = 0$ as more probable than $k = 1$.

With these modifications the equations of condition become

November Transits. Internal Contacts.

Date.	Contact.	Equation.	Weight.
1677	II.	$-0{\cdot}98V - 5''{\cdot}72 = 0$	Rej
	III.	$+0{\cdot}96V - 9{\cdot}46 = 0$	Rej
1677	II. and III.	$-0{\cdot}97V + 1{\cdot}88 = 0$	0·3
1697	III.	$+0{\cdot}75V + 0{\cdot}69 = 0$	0·3
1723	II.	$-0{\cdot}95V + 0{\cdot}97 = 0$	2·0
1736	II.	$-0{\cdot}51V + 0{\cdot}20 = 0$	1·0
	III.	$+0{\cdot}47V - 0{\cdot}65 = 0$	1·0
1743	II.	$-0{\cdot}81V + 0{\cdot}21 = 0$	1·0
	III.	$+0{\cdot}84V - 0{\cdot}18 = 0$	1·5
1769	II.	$-0{\cdot}90V - 0{\cdot}43 = 0$	1·0
	III.	$+0{\cdot}88V - 0{\cdot}53 = 0$	0·2
1782	II.	$-0{\cdot}23V + 0{\cdot}11 = 0$	3·0
	III.	$+0{\cdot}19V - 1{\cdot}13 = 0$	3·0
1789	II.	$-0{\cdot}87V + 0{\cdot}80 = 0$	2·0
	III.	$+0{\cdot}90V - 1{\cdot}07 = 0$	1·0
1802	III.	$+1{\cdot}00V - 0{\cdot}63 = 0$	3·0
1822	II.	$-0{\cdot}46V + 0{\cdot}26 = 0$	0·5
	III.	$+0{\cdot}51V + 0{\cdot}55 = 0$	1·0

Date.	Contact.	Equation.	Weight.
1848	II.	$-0.99V - 0''.61 = 0$	5·0
	III.	$+0.98V + 0.70 = 0$	0·3
1861	II.	$-0.75V + 0.26 = 0$	0·7
	III.	$+0.72V + 0.76 = 0$	5·0
1868	II.	$-0.62V + 5.90 = 0$	0·5
	III.	$+0.66V + 1.72 = 0$	6·0
1881	II.	$-0.96V - 4.34 = 0$	3·0
	III.	$+0.97V + 3.40 = 0$	3·0

May Transits. Internal Contacts.

Date.	Contact.	Equation.	Weight.
1740	II.	$-0.29W - 1''.76 = 0$	Rej
1753	III.	$+0.97W + 0.47 = 0$	3·0
1786	II.	$-0.64W + \left\{\begin{matrix} 3.59 \\ 1.19 \end{matrix}\right\} = 0$	Rej
	III.	$+0.73W + 1.60 = 0$	4·0
1799	II.	$-0.95W + 0.00 = 0$	3·0
	III.	$+0.90W - 0.28 = 0$	4·0
1832	II.	$-0.83W + 1.47 = 0$	6·0
	III.	$+0.89W - 1.16 = 0$	6·0
1845	II.	$-0.84W + 1.55 = 0$	8·0
	III.	$+0.77W - 1.76 = 0$	8·0
1878	II.	$-0.94W + 1.24 = 0$	12·0
	III.	$+0.97W - 1.34 = 0$	8·0

Professor Newcomb, who had no grounds for introducing terms proportional to the square of the time into his solution, obtains

$$V = -0''.90 - 2''.63T$$
$$W = +0.84 + 1.84T$$

I find

$$V = +0''.28 - 3''.61T - 3''.30T^2$$
$$W = +1.12 + 2.36T - 2.81T^2$$

T being measured from 1820 in units of a century.

The meanings of V and W are

$$V = 1.487\delta\lambda - 0.487\delta\pi - 1.137\delta e$$
$$- 1.01\delta\lambda' + 1.19e'\delta\pi' + 1.58\delta e'$$
$$W = 0.716\delta\lambda + 0.284\delta\pi + 0.896\delta e$$
$$- 0.97\delta\lambda' - 1.11e'\delta\pi' - 1.62\delta e'$$

where $\delta\lambda$, $\delta\pi$, δe represent the corrections required by the tabular (Le Verrier's tables) mean longitude perihelion and eccentricity of *Mercury*, and accented letters refer to the Earth.

V, W refer to November and May transits respectively. The signs of the coefficients of $\delta\pi$, δe, $\delta\pi'$, $\delta e'$ are naturally changed at the opposite side of the orbits. If we regard δe, $\delta\pi'$, $\delta e'$ as known, the equations of condition determine the two relative distances of the heliocentric positions of three points, *Mercury*, the perihelion of *Mercury*, and the Earth.

Eliminating $\delta\lambda'$ and $\delta\lambda$ successively from the values of V and W

$$\delta\lambda - 1\cdot06\delta\pi - 2\cdot79\delta e \qquad + 3\cdot16e'\delta\pi' + 4\cdot41\delta e' = -1''\cdot19 - 8''\cdot19\mathrm{T} - 0''\cdot50\mathrm{T}^2$$

$$-1\cdot07\delta\pi - 2\cdot98\delta e + \delta\lambda' + 3\cdot48e'\delta\pi' + 4\cdot92\delta e' = -2''\cdot04 - 8''\cdot48\mathrm{T} + 2''\cdot53\mathrm{T}^2$$

On the further assumption that the secular term on the right hand of the second equation is to be attributed to $\delta\lambda'$, we have

$$\delta\lambda' = +2''\cdot53\mathrm{T}^2$$

with similar assumptions

$$\delta\lambda = -0''\cdot50\mathrm{T}^2$$

The first result is a fair confirmation of the result from solar eclipses. The second is probably accidental.

A still better accordance with the eclipse value of the secular acceleration of the Earth is obtained by treating the secular acceleration of *Mercury* as zero in the equations of condition. In this manner we obtain from the November transits, 1677–1881

$$\delta\lambda' = +3''\cdot27\mathrm{T}^2$$

from the May transits, 1753–1878

$$\delta\lambda' = +2\cdot90\mathrm{T}^2$$

Weighting the two values in the proportion 204^2 to 125^2, or 8 to 3, the squares of the extent of the observations in time, we get

$$\delta\lambda' = +3''\cdot17\mathrm{T}^2$$

The terms proportional to T are slightly different from those obtained by Professor Newcomb, and they may be worked up by his methods, and slightly different numerical results in consequence obtained. The numerical changes, however, are small in comparison with the arbitrary assumption that δe, $e'\delta\pi'$, $\delta e'$ contain no terms proportional to the time other than those indicated by theory, thus throwing the whole observed discordance upon

$\delta\pi$. Moreover the numerical changes are small compared with the large "observed *minus* theoretical" value of $\delta\pi$, the longitude of the perihelion of *Mercury*. The motion of *Mercury's* perihelion remains anomalous, and is not shown to be the geometrical effect of ignoring secular variations in the tables.

On the Present State of Lunar Nomenclature.

By S. A. Saunder, M.A.

Some apology is due from me for again occupying the time of the Society with a subject so familiar to all selenographers as the confusion now existing in lunar nomenclature, and the inadequacy of our present system for the growing needs of selenography; but, as some recent remarks of mine* have led the Council of this Society to take a course of action which it is hoped may lead to an authoritative reconsideration of the questions involved, I have thought that a fuller statement of the difficulties might be of interest to those whose work lies in other directions, and might also lead to some useful suggestions from those who, like myself, have found themselves hampered by the want of a recognised language in which to express the results of their labours.

Our present system may be said to date from the publication of Beer and Mädler's map in 1837. In this the principal formations, such as Tycho or Copernicus, have separate names allotted to them; the smaller mountains are designated by affixing a letter to the name of some neighbouring principal formation, as Mösting A, Thebit B. But at once difficulties begin to be felt. These smaller mountains are denoted on the map only by the letters A, B . . ., and it is often far from easy to determine to which of the adjacent names this letter should be attached. Mädler was generally careful to place the letter towards that side of the object which was nearer to the named formation, but even in his map it is sometimes difficult to determine the name, and in other maps the position of the letter is no guide at all. The only safe method is to read through all that has been said in the text of *Der Mond*, or of Neison's *Moon*, under each of these headings until a description is found which applies to the mountain under consideration. This may well occupy half an hour, and sometimes three or four times as long may be spent without any result, for there are some of these lettered formations to which I have been unable to find any allusion in the text. It is frequently hard enough to identify a crater at all in a crowded region, and this further demand constitutes a considerable tax upon one's time.

As an instance of the confusion which may arise from this

* *Memoirs R.A.S.* vol. lvii. pp. 47, 48.